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A Generation Advantage for Multiplication Skill and Nonword Vocabulary Acquisition

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A Generation Advantage for Multiplication Skill and Nonword Vocabulary Acquisition

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Abstract

The generation effect is extended to skill learning and the acquisition and long-term retention of facts stored in semantic memory. In two experiments subjects were trained in either a read or generate condition. In Experiment 1, subjects performed simple and difficult multiplication problems. A generation advantage occurred only for the difficult problems. In Experiment 2, subjects learned to associate nonword vocabulary terms with common English nouns. A generation advantage occurred, and in both conditions subjects using mnemonic strategies showed superior performance. The results are explained in terms of a procedural account of the generation advantage, and the implications of this research are discussed for instructional applications.

A Generation Advantage for Multiplication Skill and Nonword Vocabulary Acquisition

The generation effect refers to the finding that people show better retention of learned material when it is self-produced, or generated, than when it is simply copied, or read. In the original study, Slamecka and Graf (1978) showed subjects word pairs following explicit production rules, such as an antonym rule. Subjects in a read condition were shown both words, which they simply read aloud (e.g., hot and cold). In a generate condition, subjects were provided the stimulus word (hot) and the first letter of its pair word (cold). The word-pairs were read aloud -- the second word in the pair being generated by the subject. This paradigm insured that the overt responses in the read and generate tasks were equated. These experiments demonstrated that an advantage for the generate condition held across a variety of production rules and retention tasks.

Since that time, although there have been some failures to obtain the generation effect (e.g., Begg & Snider, 1987; Slamecka & Katsaiti, 1987) and even some reversals of the generation effect (e.g., Jacoby, 1983; Schmidt & Cherry, 1989), the generation effect has been replicated in numerous studies using a wide variety of retention measures and stimuli. Specifically, the generation effect has been found for recognition measures (e.g., Glisky & Rabinowitz, 1985;

Jacoby, 1978), recall measures (e.g., Donaldson & Bass, 1980; McFarland, Frey, & Rhodes, 1980), confidence ratings (e.g., McElroy & Slamecka, 1982), and some implicit memory measures (e.g., Gardiner, 1988, 1989). It has been found both with single trial and multitrial learning (e.g., Gardiner, Gregg, & Hampton, 1988; Graf, 1980), with incidental as well as intentional learning (see, e.g., Nairne, 1988; Watkins & Sechler, 1988), and has been found to sustain retention intervals of up to approximately one week (e.g., Crutcher & Healy, 1989; Johnson, Raye, Foley, & Foley, 1981). Further, the generation effect has been obtained with words cued by related words (e.g., Slamecka & Fevreiski, 1983), words cued by meaningful sentences (e.g., Gollub & Healy, 1987; Graf, 1980), isolated words (e.g., Glisky & Rabinowitz, 1985; Nairne, Riegler, & Serra, 1991), nonwords (e.g., Johns & Swanson, 1988; Nairne & Widner, 1987; but also see, e.g., McElroy & Slamecka, 1982; Payne, Neely, & Burns, 1986), cue words (e.g., Greenwald & Johnson, 1989; but see, e.g., Slamecka & Graf, 1978), meaningful noun compounds (e.g., Gardiner & Hampton, 1985), meaningful bigrams (e.g., Gardiner & Hampton, 1985), computer commands (Scapin, 1982), product names (Thompson & Barnett, 1981), answers to multiplication problems (e.g., Gardiner & Rowley, 1984), unitized numbers (e.g., Gardiner & Hampton, 1985), and pictures (Peynircioglu, 1989).

The overarching goal of these studies has been to provide a theory or cognitive mechanism able to account for

the generation effect. For example, explanations have been proposed that refer to lexical or semantic activation (e.g., Graf, 1980; McElroy & Slamecka, 1982), effort or arousal (e.g., Griffith, 1976; Jacoby, 1978; McFarland et al., 1980), relational processes (e.g., Donaldson & Bass, 1980; Rabinowitz & Craik, 1986), and multiple factors (Hirshman & Bjork, 1988; McDaniel, Waddill, & Einstein, 1988). We propose here a procedural account of the generation effect. According to this account, the important factor is that the subjects engage in cognitive operations that serve to connect the target item to information stored in memory, rather than that the subjects actually generate, or produce, the target item. It is also crucial that at the time of the memory test the subjects are able to reinstate the learning procedures, or cognitive operations, that were used at study. This procedural account derives from several previous investigations. The importance of mental procedures was introduced by Kolers and Roediger (1984) on a general cognitive level. Glisky and Rabinowitz (1985) demonstrated more specifically that the memorial benefits associated with generation were enhanced by the repetition of the crucial operations. Crutcher and Healy (1989) showed that the most important factor in obtaining the generation effect for the answers to simple multiplication problems was that the subjects themselves perform the necessary mental operations, or cognitive procedures, to derive the answers. Healy, Fendrich, Crutcher, Wittman, Gesi, Ericsson, and

Bourne (1992) extended the notion of procedural reinstatement to account for a variety of empirical findings in the domain of long-term skill retention.

The first experiment of the present investigation was prompted by three previous studies, which were conducted in our laboratory. First, Fendrich, Healy, and Bourne (in press) showed college students simple single-digit multiplication problems, such as 3 x 5, to which they responded with the answer, in this case 15. Considerable decreases were found in the response time for this task as training progressed. Also virtually no forgetting of this skill was found.

Second, Crutcher and Healy (1989) found a generation advantage for the retention of the answers to simple multiplication problems. In this study, two of the conditions involved either reading or generating the answers. Afterward, subjects were asked to recall or recognize the specific answers that they had previously read or generated. Subjects recalled and recognized significantly more answers that had been generated than those that had only been read.

The third study was an unpublished preliminary investigation to the present study. Like the study done by Crutcher and Healy (1989), we were interested in examining the generation effect with simple multiplication problems. However, this study evaluated acquisition and retention of the skill itself, rather than memory for the specific

answers encountered. From the study by Fendrich et al. (in press), we already knew that repeatedly generating the answers to multiplication problems resulted in significant improvement in response times. However, we did not know whether we would find for this skill an advantage for generating relative to reading. In this experiment, we trained college students on simple single-digit multiplication problems for three separate one-hour sessions. Subjects were given a pretest before training, a posttest on the last day of training, and then after a month interval, a retention test. Training for the group of subjects in the read condition involved reading and copying the problem and the answer, and training for the group of subjects in the generate condition involved reading and copying the problem, but generating the answers. Training led to a significant improvement in response times across sessions, and this improvement in the multiplication skill was retained with very little forgetting across the one-month delay interval. However, there were no differences between the read and generate conditions.

The failure to find a generation advantage in this preliminary study is consistent with our procedural account of learning. According to this account, a critical factor leading to a generation advantage is that cognitive procedures be developed during the learning process. Multiplication is a skill for which most college students have already developed some cognitive procedures. For

simple problems, we would expect little or no change in these procedures as a function of training because they are extremely well entrenched. In fact, answer retrieval may be automatic for those problems. In contrast, most college students have not developed well-established cognitive procedures for more difficult multiplication problems with operands greater than 12. This observation leads to the prediction that a generation advantage would be found for more difficult problems because the generate condition would be more apt than the read condition to promote the formation of new cognitive procedures.

In the first experiment of our present series, we tested this prediction by comparing read and generate conditions for the training of both easy and hard multiplication problems. A generation advantage was expected only for the hard problems.

Experiment 1

Method

Subjects. Sixty-four men and women who were undergraduate students taking a class in introductory psychology participated for course credit. There were two experimental conditions (read and generate); subjects were assigned to conditions on the basis of their time of arrival for testing. Exactly half of the subjects in each condition were tested during the summer and the remaining half were tested during the subsequent fall.

Design. A 2X2X2 mixed factorial design was employed,

with one between-subjects factor, training condition (read, generate), and two within-subjects factors, test (pretest, posttest) and problem type (easy, difficult).

Apparatus and materials. Stimuli were presented either with a Zenith Data Systems or an IBM/PC computer. The Zenith computers were equipped with Zenith monitors, and the IBM/PC computers had Amdek 310 or 410 monitors. Each keypad included a label over the minus sign with an equal sign, and the computers were programmed to interpret the minus sign as an equal sign.

Ten easy and ten corresponding difficult multiplication problems were shown to the subjects. As shown in Table 1, both types of problems consisted of a two-digit multiplier followed by a one-digit multiplier. The products all consisted of three-digit answers. For the easy multiplication problems the second digit of the two-digit multiplier was always 0 (e.g., 40 x 9). For the difficult multiplication problems, the first digit was always 1 (e.g., 14 x 9). Thus, apart from the second digit of the easy problems and the first digit of the difficult problems, the multipliers remained constant for both sets of problems.

Insert Table 1 about here

Two pseudorandom orders of the 20 problems were constructed for the pretest and the posttest with the constraints that no more than two problems with the same

single-digit multiplier occurred consecutively. Each subject saw a different order on the two tests. The orders were counterbalanced across subjects in each condition so that the two orders were used equally often in both the pretest and the posttest. During training subjects were exposed to ten blocks of problems, each block consisting of a random permutation of the 20 problems. The permutation for a given block of a given subject was created by the computer at the time of training.

Procedure. Subjects were tested in small rooms with one or two computers. Each subject sat facing a computer monitor which was at eye level.

For the pretest and posttest, each problem appeared in the middle of the screen with the signal "answer:" directly below it. The subject was to type a response which appeared next to the colon. Each problem remained on the screen until the subjects entered a response and pressed the enter key. (Pressing the enter key on its own was not sufficient. Two presses of the enter key alone, without a response, caused a tone to sound, which alerted the experimenter of the error.) After pressing the enter key, the following problem appeared immediately on the screen. No feedback was provided during the tests.

For the training phase of the experiment, each problem (or, in the read condition, each equation) remained on the screen until the subject entered a response and pressed the enter key. Feedback for incorrect responses included a

1,000 ms tone followed by a 2,500 ms display of the word "incorrect" along with the correct equation; feedback for correct responses included simply a 750 ms display of the word "correct."

At the start of the session subjects were given a verbal general introduction to the experiment including, for example, the fact that the experiment would include three parts, and that the instructions for each part would be given on the computer monitor. Subjects were told that they could not correct typing errors (e.g., by using the backspace key). However, they were encouraged to complete any response even after they noticed that they had made a typing error. They were also told that they should use only the keypad (not the number row) of the terminal for their responses, and they were shown the location of the keys representing the multiplication sign (the asterisk key) and the equal sign (the minus sign key was relabeled with an equal sign). Instructions for each part of the experiment (i.e., pretest, training, and posttest) appeared on each subject's computer monitor at the beginning of each part. All subjects were given the same instructions for the pretest and posttest. The subjects were told that they were going to take a short arithmetic test with multiplication problems; they were given as an example the problem 12*7=. They were instructed to type in the answer followed by the enter key as quickly and accurately as they could and that the computer would record both their answer and their

response time.

Following the pretest subjects were given instructions for the training, with different instructions for the read and generate conditions. Subjects in the read condition were told they were going to read series of multiplication equations that would appear in the middle of the computer screen one at a time. They were told to type the problem and the answer exactly as written under each equation presented. They were again given the example 12*7=84. They were told further that as soon as they typed an answer, they should press the enter key, after which the computer would provide feedback. The subjects were also informed that they would be tested on the multiplication problems at the end of their session. Subjects in the generate condition were told that they were going to read series of multiplication problems (i.e., not full equations) and they were to type the problem exactly as it was written and type the answer (which they generated themselves). Otherwise, the instructions for the generate condition were equivalent to those for the read condition. At the end of each of the ten blocks of training subjects were given the opportunity for a short break, with the instructions to press the return key twice when they were ready to continue.

Results and Discussion

Accuracy. The results are summarized in Table 2 in terms of proportions of correct responses as a function of training condition (read, generate), problem type (easy,

difficult), and test (pretest, posttest).

Insert Table 2 about here

There was an overall improvement in accuracy from the pretest (*M*= .783) to the posttest (*M* = .837), $F(1, 62) = 9.8$, *MSe* = 0.0189, $p = .003$, and easy problems (*M* = .900) yielded higher accuracy overall than difficult problems (*M* = .720), $F(1, 62) = 70.9$, *MSe* = 0.0294, $p < .001$. Most crucially, an advantage for generate training was only found on the difficult problems after training (i.e., in the posttest); the three-way interaction of training condition, test, and problem type was significant, $F(1, 62) = 5.9$, *MSe* = 0.0090, $p = .017$, as was the two-way interaction of training condition and test, $F(1, 62) = 5.0$, *MSe* = 0.0189, $p = .028$, as well as the two-way interaction of training condition and problem type, $F(1, 62) = 4.9$, *MSe* = 0.0294, $p = .028$.

Separate analyses of variance were conducted on the data from each problem type. For the easy problems, there was only a main effect of test, $F(1, 62) = 10.4$, *MSe* = 0.0087, $p = .002$; the interaction of training condition and test was not significant, $F(1, 62) < 1$. In contrast, for the difficult problems, there was both a main effect of test, $F(1, 62) = 5.0$, *MSe* = 0.0193, $p = .028$, and an interaction of training condition and test, $F(1, 62) = 7.5$, *MSe* = 0.0193, $p = .008$.

<u>Response latency</u>. Correct response latencies in seconds, reflecting the time to press the first digit of the answer, were tranformed by a log to the base 10 function. An analysis of variance was conducted on the mean log response latency for each subject as a function of problem type and test. Three subjects were excluded from the analysis (two from the generate condition and one from the read condition) because they made no correct responses for the difficult problems on the pretest. The resulting mean response latencies (in log s) are summarized in Table 3 as a function of training condition, problem type, and test.

Insert Table 3 about here

There was an overall decline in response latency from the pretest ($\underline{M}$= .712 log s) to the posttest ($\underline{M}$ = .495 log s), $\underline{F}(1, 59) = 338.0$, $\underline{MSe} = 0.0085$, $\underline{p} < .001$, and easy problems ($\underline{M}$ = .414 log s) yielded lower response latencies overall than difficult problems ($\underline{M}$ = .792 log s), $\underline{F}(1, 59) = 432.2$, $\underline{MSe} = 0.0201$, $\underline{p} < .001$. In addition, there was an overall improvement due to generation training; that is, the interaction of training condition and test was significant, $\underline{F}(1, 59) = 7.6$, $\underline{MSe} = 0.0085$, $\underline{p} = .008$. The generation advantage appears larger for the difficult problems than for the easy problems, but the three-way interaction of training condition, test, and problem type did not reach standard levels of significance, $\underline{F}(1, 59) = 2.8$, $\underline{MSe} = 0.0073$, $\underline{p}$ =

.100.

Separate analyses of variance were conducted on the data from each problem type. For the easy problems, there was only a main effect of test, $F(1, 59) = 271.3$, $MSe = 0.0062$, $p < .001$; the interaction of training condition and test was not significant, $F(1, 59) = 1.0$. In contrast, for the difficult problems, there was both a main effect of test, $F(1, 59) = 126.4$, $MSe = 0.0097$, $p < .001$, and an interaction of training condition and test, $F(1, 59) = 8.2$, $MSe = 0.0097$, $p = .006$.

Summary. In summary, in this experiment we found an advantage due to generate training for hard multiplication problems, but not for easy problems, both for response latency and accuracy. The difference between easy and hard problems that we consider to be most important is that before training the subjects already have well established cognitive procedures for the easy problems but not for the hard problems. With training the generate condition should be more apt than the read condition to promote the formation of the new cognitive procedures needed to solve the hard problems.

Unlike most previous investigations of the generation effect, this study showed a generation advantage for a skill requiring access to facts, or knowledge residing in semantic memory, rather than for events, or information stored in episodic memory. The facts studied in this experiment were already known by the subjects before training, although the

subjects presumably developed new cognitive procedures for efficient retrieval of those facts in the case of the hard problems. Of great interest would be the extension of this investigation to the situations in which individuals are learning new facts. Can a generation advantage be found in those situations? Such a question has important educational implications because most work in the classroom involves teaching new material, rather than improving the efficiency with which old material is retrieved from semantic memory.

Therefore, in our second experiment we extended our comparison of the read and generate training conditions to the learning of new material. For this purpose, we used verbal material instead of arithmetic calculations.

Experiment 2

In Experiment 2 we taught subjects word-nonword associations, under the cover story that they were learning foreign vocabulary items. Our procedural account of learning and the findings from our first experiment led us to predict that the use of cognitive procedures would aid learning and retention. However, we did not directly assess the cognitive procedures used in the first experiment. Hence, in the present experiment we directly examined the procedures used by the subjects. The most probable relevant cognitive procedures in this case would be mnemonic codes linking the word and nonword components of each pair. We expected subjects in the generate condition to develop more mnemonic codes than subjects in the read condition and,

therefore, to show superior learning and retention of the word-nonword pairs. We further expected that subjects in the read condition who did develop mnemonic codes would show a level of performance comparable to that of subjects in the generate condition. To evaluate the long-term impact of both generate training and mnemonic coding, we included a retention test after a one-week delay.

Our prediction of a generation advantage for the learning of word-nonword associations may at first seem misguided because many early studies failed to find the generation effect with nonwords or other meaningless responses (see, e.g., Gardiner & Hampton, 1985; Graf, 1980; McElroy & Slamecka, 1982; Nairne, Pusen, & Widner, 1985; Payne et al., 1986). However, more recently, Nairne and Widner (1987) showed that a generation effect could be obtained with nonwords when the retention test provided a stimulus context appropriate to (i.e., consistent with) the training context. Further, Johns and Swanson (1988) demonstrated that a generation effect can be obtained with nonwords when the subjects are shown the entire nonword stimuli via feedback. Moreover, although Nairne et al. (1985) did not report a generation effect in their Experiment 1, which involved testing over repeated trials, they did obtain a significant interaction of trials and the read/generate variable, consistent with the observation that a generation advantage was obtained by the last trial. In the present experiment we used an appropriate stimulus

context at test, provided the entire nonwords via feedback, and examined the generation advantage at the end of series of training trials. Hence, our prediction of a generation advantage under these conditions does not seem farfetched given that we utilized methods previously found to be successful in obtaining a generation effect with nonwords.

Method

Subjects were given a ten-minute initial study period to become familiar with the word-nonword pairs followed by a pretest, 14 blocks of training, and then a posttest; one week later a retention test was administered. To assess the extent of mnemonic coding, after the retention test we administered a retrospective questionnaire asking the subjects to report their use of mnemonic codes for each word-nonword pair.

Subjects. Twenty-four men and women who were undergraduate students taking a class in introductory psychology participated for course credit. There were two experimental conditions (read and generate); subjects were assigned to conditions on the basis of their time of arrival for testing. Exactly half of the subjects in each condition were tested during the summer and the remaining half were tested during the subsequent fall.

Design. A 2X2 mixed factorial design was employed, with one between-subjects factor, training condition (read, generate), and one within-subjects factor, test (pretest, posttest, retention test).

Apparatus and materials. As in Experiment 1, stimuli were presented either with a Zenith Data Systems or an IBM/PC computer. The Zenith computers were equipped with Zenith monitors, and the IBM/PC computers had Amdek 310 or 410 monitors.

Thirty word-nonword pairs were constructed. The English words were all single-syllable nouns three to six (*M* = 4.47) letters in length. The English words were all frequent according to the Kucera and Francis (1967) norms; the minimum frequency (out of approximately 1 million words of text) was 67, the maximum was 2,316, and the mean was 540. The corresponding nonwords were all pronouceable single syllables, beginning and ending with a consonant, three to five (*M* = 4.17) letters in length. The nonwords were paired with the words in such a way as to minimize obvious mnemonic links. The pairs are shown in Table 4.

Insert Table 4 about here

Three random orders of the 30 English words were constructed. Each subject was shown a different one of these orders at each of the three tests (pretest, posttest, and retention test). Across subjects in each condition each of the six permutations of the three orders was used twice.

Subjects were exposed to 14 blocks of training, 4 blocks on the first day and 10 blocks on the following day, each block consisting of a random permutation of the 30

English words. The permutation for a given block of a given subject was created by the computer at the time of training.

Procedure. Subjects were tested in small rooms with one or two computers. At the start of the first session subjects were given a typewritten list of English word-nonword pairs to study. They were told that the nonword was from a foreign language with the equivalent meaning of its English word mate. They were also told that they would be learning these foreign words throughout the course of the study. They were given ten minutes to study the pairs any way they wished but without using paper and pencil. It was suggested to the subjects that they begin by reading over all of the pairs at least once.

Following the ten-minute initial study period, subjects were given a pretest sheet of paper with each English word followed by a blank line. They were told to write down as many of the foreign words as they could remember next to their English word equivalents. They were also encouraged to guess and to try not to leave blanks, but they were allowed to do so if necessary. The same procedure and instructions were employed for the posttest following training and the retention test one week later.

During the training period each subject sat facing a computer monitor which was at eye level. They were given written instructions appropriate for their training condition (read or generate). They were also provided answer sheets with two or three blanks per line, depending

on the condition. Subjects in the read condition were reminded that they had just learned a list of foreign words with their English equivalents and that the English words would be presented on the computer screen. They were instructed to write the English word on the answer sheet in the first blank after which they were to press the space bar which caused the foreign word to be presented on the screen. The subjects were instructed to copy the foreign word in the second blank (i.e., next to the English word). After they had finished writing the foreign word down, they were instructed to press the space bar to begin the next word-nonword pair. After the complete list of 30 word-nonword pairs was presented, there was a short break, after which the subjects were to press the space bar twice, and then the next list of pairs was presented. The subjects were required to use the same hand to press the space bar as they used to write down the words. This requirement insured that they wrote down the words only after they had seen them on the computer screen, as would be necessary for a read condition.

The generate condition was identical to the read condition apart from the instructions given to the subject. After writing down the English word, the subjects were instructed to write down the foreign word, which was <u>not</u> shown to them on the computer screen. The subjects were required to write something in the second slot whether they were certain or not. After writing both the English word

and the foreign word, the subjects were to press the space bar. At that point the foreign word was displayed on the computer screen, and subjects were told to copy that word in the third blank only if they had not written it correctly on their first try (i.e., in the second blank).

Following the retention test the subjects were given a sheet of paper containing a brief questionnaire. The subjects were provided an explanation and an example of what constituted a mnemonic strategy for learning the word-nonword pairs. Specifically, they were told: "Sometimes when people want to learn and remember something they use some kind of strategy or mnemonic to link what they are learning to something that they already know." As an example, for a hypothetical word-nonword pair "lion-dlim," they word given the mnemonic code "the lion in the dlim." The word-nonword pairs were listed on a second sheet of paper in the same order shown to subjects during the initial study period. Subjects were told to indicate by writing "yes" or "no" beside each pair whether they had employed some strategy or mnemonic as a means to learn that pair. Whenever they wrote "yes," they were also to describe the mnemonic in detail. If they could not recall the mnemonic but were sure that they had used one, they were told to write down that they did not remember the mnemonic. If they remembered using more than one code for a particular pair, they were to write them all down.

Results and Discussion

Accuracy. A nonword was scored as correct on a test only if it was placed beside the appropriate English word. Misspellings were allowed if pronunciation of the nonword was preserved. Two separate scorers tabulated all the data; any discrepancies between the scorers were resolved after discussion. The results are summarized in Table 5 in terms of proportions of correct responses as a function of training condition (read, generate) and test (pretest, posttest, retention).

Insert Table 5 about here

There was a main effect of test, $F(2, 44) = 118.3$, $MSe = 0.0171$, $p < .001$, reflecting both an overall improvement in accuracy from the pretest ($M = .325$) to the posttest ($M = .894$) as well as forgetting from the posttest to the retention test ($M = .707$). Single-degree of freedom tests show that the difference between the pretest and both the subsequent tests is significant, $F(1, 22) = 155.7$, $p < .001$, as well as the difference between the posttest and the retention test, $F(1, 22) = 38.7$, $p < .001$. As expected, the generation effect was only evident after the pretest; the interaction of training condition and test was significant, $F(2, 44) = 3.3$, $MSe = 0.0171$, $p = .048$. Crucially, the single-degree of freedom tests showed that whereas there was an interaction of training condition and test, comparing the

pretest and both subsequent tests, $F(1, 22) = 4.7$, $p = .041$, there was not an interaction of training condition and test, comparing the posttest and retention test, $F(1, 22) < 1$.

In order to pinpoint the locus of the generation advantage, we used the mnemonic score from each subject as a covariate. On the basis of the mnemonic strategy questionnaire, a mnemonic score was given to every word-nonword pair for each subject. The mnemonic score was 0 for a "no" response, 1 for a "yes" response with either an indication that the subject did not remember the mnemonic or only a description of a phonetic or graphemic mnemonic, and 2 for a "yes" response with a semantic mnemonic. The subject's total mnemonic score was simply the mean of the 30 scores for each word-nonword pair. Separately two individuals determined the mnemonic score for every subject. In the few cases in which the individuals did not agree on the mnemonic score, any discrepancies were resolved after discussion. There was no difference between the generate ($M = 1.150$) and read ($M = 1.156$) training conditions in terms of the mnemonic scores. Controlling for mnemonic scores in the analysis of covariance, the crucial interaction of training condition and test, comparing the pretest and both subsequent tests, remained significant, $F(1, 21) = 6.0$, $p = .023$. In addition, controlling for training condition, there was an interaction of mnemonic score and test, comparing the pretest and both subsequent tests, $F(1, 21) = 6.9$, $p = .016$, suggesting that mnemonic coding aided

performance after training.

There was in addition a marginally significant three-way interaction of condition, mnemonic score, and test, comparing the pretest and both subsequent tests, $\underline{F}(1, 20) = 3.3$, $\underline{p} = .082$. To illustrate this relationship of mnemonic score to both test and training condition, we transformed the mnemonic score into a categorical variable by means of a median-split procedure. (The analysis of covariance employed mnemonic score as a continuous variable, reflecting each subject's use of a mnemonic strategy for all of the 30 word-nonword pairs.) The median-split procedure categorized the subjects in each training condition into those with a relatively low and those with a relatively high mnemonic score. Table 6 presents the proportions of correct responses as a function of mnemonic score category (low, high), training condition, and test.

Insert Table 6 about here

On the basis of our procedural account, we expected that subjects would be aided by the formation of a mnemonic code, or semantic association, linking the word to its corresponding nonword. We predicted, first, that more subjects in the generate condition than in the read condition would use mnemonic coding. This prediction was not confirmed. Second, we predicted that those subjects in the read condition who used mnemonic coding would show a

level of performance comparable to that shown by subjects in the generate condition. This prediction was verified. As is clear from Table 6, subjects in the read condition who are high on mnemonic coding are indistinguishable in their level of performance on the posttest and retention test from subjects in the generate condition.

Item differences. The analyses reported above showed that subjects who used a high degree of mnemonic coding demonstrated superior performance relative to subjects who used a lower degree of mnemonic coding. A related question pertains to differences among the items rather than differences among the subjects. Does the likelihood of recalling a particular nonword depend upon whether subjects employed a mnemonic code for that item? To answer this question, we conducted an analysis of the proportion of correct responses dividing the items for each subject into those given a 0 (i.e., no mnemonic code), a 1 (i.e., a nonsemantic mnemonic code), or a 2 (i.e., a semantic mnemonic code). For those cases in which a subject had no items in a particular scoring category, we replaced that missing proportion with the mean from the subjects in the same training condition (i.e., read or generate) and at the same test (i.e., pretest, posttest, or retention test). Table 7 presents the mean proportions of correct responses as a function of mnemonic score (0, 1, 2), training condition, and test.

Insert Table 7 about here

A mixed factorial analysis of variance was conducted on these data including the single between-subjects factor of training condition and the two within-subjects factors of test and mnemonic score. As found in the previous analyses, there was a significant main effect of test, $F(2, 44) = 96.0$, $MSe = 0.0661$, $p < .001$ and a significant interaction of training condition and test, $F(2, 44) = 4.0$, $MSe = 0.0661$, $p = .025$. Of most interest, there was also a significant main effect of mnemonic score, $F(2, 44) = 7.7$, $MSe = 0.0679$, $p = .002$. Overall, the proportion of correct responses was highest for the items given a score of 2 ($M = .714$), next highest for the items given a score of 1 ($M = .625$), and lowest for the items given a score of 0 ($M = .543$). There were no significant interactions involving mnemonic score. This finding suggests that the advantage for the high mnemonic score did not depend on either test or training condition. However, there appears to be a retention advantage (i.e., less of a difference between the posttest and the retention test) for a semantic mnemonic (i.e., a score of 2) relative to both a nonsemantic mnemonic (i.e., a score of 1) and the absence of a mnemonic (i.e., a score of 0). To establish the statistical reliability of this finding, we conducted a separate analysis of variance containing only the posttest and retention test. That

analysis revealed a significant main effect of test, $F(1, 22) = 38.1$, $MSe = .0573$, $p < .001$, reflecting forgetting across the retention interval, along with a main effect of mnemonic score, $F(2, 44) = 6.5$, $MSe = .0684$, $p = .004$, reflecting increased performance as the mnemonic score increased. Most importantly, there was also a significant interaction of mnemonic score and test, $F(2, 44) = 3.9$, $MSe = .0256$, $p = .027$, in agreement with the observation that forgetting across the retention interval is least for a semantic mnemonic code.

General Discussion

Summary. We have found a generation advantage in two experiments examining the acquisition and retention of facts, or knowledge residing in semantic memory. This effect was found even though training condition was varied in a between-subjects design (cf. Begg & Snider, 1987; Slamecka & Katsaiti, 1987) and even though the to-be-remembered information consisted of nonwords in Experiment 2 (cf. McElroy & Slamecka, 1982; Nairne et al., 1985; Payne et al., 1986). No previous investigators have found a generation advantage for nonwords in a between-subjects design. We explained the generation advantage we found in terms of a procedural account of memory (see, e.g., Crutcher & Healy, 1989; Healy et al., 1992), according to which the critical factor leading to a generation advantage is that cognitive procedures be developed during the learning process and that these

procedures be reinstated at test. This account led us to predict that the generation advantage would be found for difficult but not easy multiplication problems and that the generation advantage would be eliminated for subjects who use mnemonic codes in vocabulary acquisition. We found support for both of these predictions.

Alternative explanations of the generation effect. Three classes of theoretical explanations of the generation effect have received the most attention: (a) those attributing the effect to the amount of effort involved, (b) those proposing the necessity of semantic activation, and (c) those emphasizing the relationship between the cue and the target.

Some researchers have supposed that the generation effect is due to increased amount of effort (e.g., Griffith, 1976; McFarland et al., 1980) or arousal (Jacoby, 1978). This proposal emphasizes the process of generation itself. Indeed, many researchers have found what is called an effort effect, wherein the more "effort" that is expended during encoding, the better the subsequent recall of the encoded items (e.g., Eysenck & Eysenck, 1979; Griffith, 1976; Jacoby, Craik, & Begg, 1979; Kolers, 1973; 1975; Tyler, Hertel, McCallum, & Ellis, 1979; see also Mitchell & Hunt, 1989, for a review of the literature). Effort, in these studies is generally operationalized in terms of the difficulty of the task (e.g., Jacoby, 1978) or the amount of cognitive processing resources required (e.g., Griffith,

1976). According to effort hypotheses of the generation effect, it is the increased effort associated with generating a response that results in superior performance on retention tasks. There are several problems associated with any hypothesis that the amount of effort used for a task has memorial consequences. The first concerns the difficulty of defining or operationalizing the construct of "effort." Once defined, a subsequent problem is the difficulty of isolating the amount of effort used for a task and keeping constant all other variables that can affect later recall. One of the most important confounding variables is the amount of time spent on a task. It is difficult, and perhaps impossible, to specify tasks that require equal amounts of time, and yet varying degrees of effort. A common solution to that problem is to require subjects to spend equal amounts of time on all tasks. That solution unfortunately leaves open the question of what cognitive processing is actually occurring during the excess time for the easier tasks. That is, although subjects may spend the same amount of time completing both a hard and an easy task, there is no guarantee that after the easy task is completed the extra time is actually allotted to processing the information in the task (they may be thinking about what to have for dinner). However, despite the methodological and theoretical problems associated with the concept of effort, it remains intuitively appealing as a partial explanation of why the generation effect occurs. Although

generation effects have been found for trivial tasks requiring virtually no effort at all (e.g., Glisky & Rabinowitz, 1985), in most cases the task of generating is simply more difficult (and thus more effortful) than the task of reading. Nevertheless, effort has not been isolated as the primary factor leading to the generation effect.

The second class of explanations includes those proposing the necessity of semantic activation. There have been two major lines of research directed at determining the role of semantic processing in the generation effect. One line involved the manipulation of the meaningfulness of the generated item. Initial findings indicated that the generation effect did not occur with meaningless items, such as nonwords (e.g., McElroy & Slamecka, 1982), anomalous sentences (Graf, 1980), or meaningless bigrams (Gardiner & Hampton, 1985). However, further exploration of the issue has indicated that as long as the subject is tested on the same items as presented at test, the generation effect occurs regardless of the nature of the generated item, in accord with the principle of test appropriateness (e.g., Nairne & Widner, 1987). Another line of research was directed at the distinction between implicit and explicit memory, and the supposition that the generation effect was solely the enhancement of explicit memory and thus involved only conceptual processing. Initial findings supported this view (e.g., Jacoby, 1983). However, when Gardiner (1988, 1989) equated the conditions of study and test in an

implicit memory paradigm, the generation effect reappeared.

Some researchers have hypothesized that the generation effect is due to the enhancement of the relationship between the cue and the target (e.g., Donaldson & Bass, 1980; Rabinowitz & Craik, 1986). Donaldson and Bass (1980) suggested that the act of generating resulted in a superior encoding of the cue-target relationship and that the factor underlying the generation effect is that the subject perform a check on each generated target to ensure that the response adequately meets its prescribed relation to the stimulus. They found that a read task which also required the evaluation of the goodness of the relationship resulted in a memorial advantage for the target items similar to that found for a generate task. This result is similar to our finding that the retention of the subjects in the read condition who developed mnemonics when learning word-nonword pairs resembled that of the subjects who had generated the nonword. Some researchers have argued against the importance of the relationship between the cue and the target on the basis that no memorial advantage is found for the cues in the generate task (Slamecka & Graf, 1978). Other researchers, however, have found generation effects for cues (Greenwald & Johnson, 1989). On the other hand, Glisky and Rabinowitz (1985) found a generation effect when single words were generated from word fragments; this result cannot be easily explained in terms of relational processing. These contradictory findings leave this issue

unresolved, although it seems clear that relational factors are not sufficient to explain the generation effect.

Procedural account. It is evident that there have been empirical findings both supporting and contradicting the three classes of explanations reviewed above. We believe that it is useful to view the procedural account, not as an alternative to the above explanations, but rather as consistent with them. The procedural account emphasizes the importance of procedures instead of semantic activation, effort, or relational factors.

Our procedural account, to avoid circularity, relies on our having a clear and consistent definition of what constitutes a cognitive procedure. For this account to lead to specific predictions and new insights, it is necessary that attempts be made to define and operationalize more precisely the concepts of procedures and proceduralization. In this study, we have not attempted to provide a definition of a cognitive procedure that would cover all tasks and domains. In our first experiment, it was not necessary for us to provide an operational definition for a cognitive procedure relevant to mental multiplication because we relied on the assumption that new cognitive procedures would be more likely to be developed for the difficult than for the easy multiplication problems. This assumption seems reasonable even without specifying in precise terms what procedures are being employed by the subjects to perform the arithmetic calculations. In contrast, in our second

experiment we did employ a clear-cut operational criterion for what constituted a cognitive procedure. The criterion we used -- that a mnemonic code be developed -- is not meant to apply to all tasks. Nor is it meant to provide an exclusive definition for the vocabulary learning task we employed. But it did allow us to identify a specific type of procedure that was found to promote superior acquisition and long-term retention.

Educational implications. As we mentioned earlier, we think that these findings have important educational implications. Previous studies of the generation effect have been limited almost exclusively to examinations of events stored in episodic memory, whereas the present study has examined facts stored in semantic memory. Most work in the classroom involves teaching information that is to be stored in semantic memory, so our finding a generation advantage for this type of information implies that classroom teaching would benefit by encouraging students to generate the to-be-learned material.

Indeed, there is a current trend in elementary schools to teach children how to use calculators for solving multiplication problems, instead of requiring them to generate answers using the multiplication table. The study we have presented today (which included difficult multiplication problems not already memorized by our adult subjects and new foreign vocabulary items) suggests that children, when learning new multiplication problems, should

not use calculators but rather should perform the multiplication operations mentally. Generating the answers to the problems, instead of simply reading them from the calculator display, should lead to optimal acquisition and long-term retention. More generally, our findings point to the important implications of the generation effect in the applied realm outside the laboratory. Although this effect has been widely investigated in the laboratory, there has been little attempt to consider the possible applications of the generation effect to the classroom or other real-world settings. Our findings indicate that future applied research on the learning and retention of skills and facts would benefit from use of the generation paradigm. Ultimately, extending the generation effect to instructional settings may enlighten our understanding of the factors underlying the generation effect, particularly the critical role of proceduralization.

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Table 1

Multiplication Problems Used in Experiment 1

Easy Problems	Difficult Problems
40*9 = 360	14*9 = 126
60*7 = 420	16*7 = 112
60*8 = 480	16*8 = 128
70*7 = 490	17*7 = 119
70*8 = 560	17*8 = 136
80*8 = 640	18*8 = 144
80*9 = 720	18*9 = 162
90*7 = 630	19*7 = 133
90*8 = 720	19*8 = 152
90*9 = 810	19*9 = 171

Table 2

Proportions of Correct Responses in Experiment 1 as a Function of Training Condition, Test, and Problem Type.

	Test			
	Pretest		Posttest	
Condition	Easy	Difficult	Easy	Difficult
Read	.881	.681	.925	.669
Generate	.866	.703	.928	.825

Table 3

Mean Correct Response Latencies (log seconds) in Experiment 1 as a Function of Training Condition, Test, and Problem Type.

	Test			
	Pretest		Posttest	
Condition	Easy	Difficult	Easy	Difficult
Read	.520	.887	.299	.737
Generate	.543	.898	.294	.646

Table 4

Word-Nonword Pairs Used in Experiment 2

Word	Nonword
box	shem
year	kril
hand	bruk
school	cron
time	plic
work	squiv
house	tralt
child	wath
part	spem
place	hirg
heart	dront
field	vour
month	grat
light	yord
rate	baz
job	lerb
home	skal
mile	vlat
day	swib
view	dword
arm	trin
food	prug
peace	blent
fire	zwird
street	flirn
game	slif
club	tob
floor	cruf
bed	gult
spring	raub

Table 5

Proportions of Correct Responses in Experiment 2 as a Function of Training Condition and Test.

	Test		
Condition	Pretest	Posttest	Retention
Read	.353	.833	.658
Generate	.297	.956	.756

Table 6

Proportions of Correct Responses in Experiment 2 as a Function of Mnemonic Category, Training Condition, and Test.

	Test		
Mnemonic Category	Pretest	Posttest	Retention
Low			
Read	.283	.667	.517
Generate	.294	.944	.706
High			
Read	.422	1.000	.800
Generate	.300	.967	.806

Table 7

Proportions of Correct Responses in Experiment 2 as a Function of Mnemonic Score, Training Condition, and Test.

	Test		
Mnemonic Score	Pretest	Posttest	Retention
No mnemonic (0)			
Read	.336	.743	.482
Generate	.197	.887	.615
Nonsemantic mnemonic (1)			
Read	.367	.916	.655
Generate	.200	1.000	.610
Semantic mnemonic (2)			
Read	.430	.908	.752
Generate	.361	.984	.846